

THE
CLIMATE

OF THE
ISLAND OF MADEIRA,

AND THE
ERRORS AND MISREPRESENTATIONS OF SOME RECENT
AUTHORS ON THIS SUBJECT,

CONSIDERED IN
A LETTER

ADDRESSED TO
GEORGE LUND, M.D.

BY
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MDCCCLIV.

ON
THE CLIMATE OF MADEIRA.

Funchal, Madeira,
1 June, 1854.

DEAR DR. LUND,

I have undertaken to put down upon paper some of my ideas on the subject of Medico-meteorology, with reference particularly to the climate of Madeira. You, who know my taste for scientific pursuits, may probably imagine that I am much better qualified than I am, in fact, to furnish you with some valuable hints. In the first place, my knowledge of medicine and of diseases is as limited as is well possible, since it is confined to that which I have unavoidably acquired during some years of experience—not as a physician, but as a patient. Of this, however, you cannot be entirely ignorant. Secondly, wherever I have given my attention to medico-meteorological writings, I have been struck with what appears to me to be the extremely unscientific manner in which the indications of meteorological instruments are dealt with, and this has created in me a distaste for the pursuit of a science which I can scarcely hope to advance. It appears to me, that the value and use of such indications are so imperfectly understood, that they have yet to be ascertained, by careful and laborious observations made in localities the effects of the climate of which, in originating, aggravating, and alleviating, particular diseases are known by independent means; but, that the generality of persons who have *written* on the subject, seem to think that a few hygrometric and thermometric data are sufficient to enable them to pronounce that a climate of long-established reputation, whether good or bad, is in fact the

reverse of what was previously supposed. Moreover, it seems to be a matter of utter indifference, that such data are often obtained from extremely defective instruments, observed by persons whose habits and pursuits have not qualified them for the task ; and who can scarcely be supposed to have had sufficient leisure to enable them to devote to it the degree of laborious and minute attention that it requires. But so it is, that persons who choose arrogantly to adopt and advocate any peculiar views upon a subject which is sufficiently obscure to admit of being so dealt with, and who are not seeking philosophically to investigate the truth, require only data upon which to found their arguments ; and, whether those data are true or false, is to them a matter of too little importance to be worth the trouble of investigation.

What the climate of Madeira really is, seems to be an interesting and much-disputed question. I fancy that the opinions of the medical men of the present day, in England, are founded in great measure, either directly or indirectly, upon Dr. Mason's *Treatise on the Climate and Meteorology of Madeira* ; and that certain advocates of peculiar views have, by one-sided quotations, contributed, in no small degree, to make that book convey impressions very different from what the author intended ;—to say nothing of the high estimation in which Dr. Mason's observations seem to be regarded by those persons, and the very little credit which is given to them here, not by medical men alone, but by all persons whom I have heard speak of them, and whose knowledge of meteorology in general, or of Dr. Mason's observations in particular, render their opinions of any value.

The editor of Dr. Mason's book, page 199, suggests, that if visitors to Madeira would employ a part of their leisure in recording the state of the weather, the difference of opinion, now (1850) existing, as to the advantages or disadvantages of its climate, in certain cases of disease, would disappear. I cannot help thinking such records would contain a large mass of contradictory evidence, calculated to be useful to advocates of any views whatever, except the truth. The suggestion, however, is useful as an admission of the insufficiency of Dr. Mason's observations to settle those differences of opinion. It also furnishes a remarkable instance of implicit and exclusive faith in metcoro-

logical data, as means of determining the *effects* of a climate upon diseases; for the editor apparently does not think, that medical reports would be necessary or even useful adjuncts to such records. The same gentleman (page 200) also suggests that the medical practitioners resident in the island can hardly allow the subject to be longer neglected, in the present advanced state of science, without subjecting themselves to the reproach of indifference, relative to the charge sometimes urged against them, of withholding the truth, under a dread that the far-famed climate of the island will not bear the test of close and accurate examination. This reads to me something like charging a man with not looking at his barometer on a fine day under a dread that its index will point to "foul weather." (And, by way of parenthesis, let me ask, why should the medical practitioners be charged with withholding that which is patent to every one who chooses to investigate the subject, and for which investigation the editor seems to think every invalid visitor is sufficiently well qualified?) But, in truth, we have long since learnt that those amusing little indications which instrument makers still continue to put into the mouth of the barometer, are not to be trusted; and I should have thought that, by this time, the hygrometer need not be similarly falsified. The barometer is a very valuable instrument; but, as has been justly observed, it has been brought into disrepute by the absurdity of engraving the words "fair, foul, wet, dry," etc., at different parts of the scale; and really one would suppose that some of our medical men are prepared to write the words "consumption, fever, cholera," etc., at different points of the hygrometric scale, and to prescribe climates for their patients accordingly. And here I must quote from the *Athenæum* for 1853, page 248, the following extract from a recent publication, by T. H. Burgess, M.D., which the *Athenæum* gives as the summing up of the author's experience; for this article has led to the present discussion:—

"It results, from the preceding statements, that much misconception prevails with respect to the efficacy of *foreign* climates in cases of pulmonary consumption; and, however agreeable to the senses warm air, sunny skies, and luxuriant vegetation may seem, they afford no proof of salubrity, nor of the beneficial

effects of any climate. Madeira, with all its sanitary fame, is no exception to this rule, as the meteorological observations of Drs. Heineken, Gourlay, and Mason incontestably establish. Malta, etc., etc.”—In the name of common sense, how can meteorological observations incontestably establish any such fact? Do the numerical readings of the barometer, hygrometer, etc., or the points of the compass towards which the wind-vane happens to point, afford proof of the salubrity, or of the *beneficial effects* of climate any better than what is agreeable to the senses? Taking this as a specimen, I can easily believe that it does result from the “preceding statements,” referred to in the above quotation, that much misconception prevails *somewhere*; for it seems that, because we are told that the hygrometers of the above-named observers gave certain numerical results, we must, without further inquiry, either respecting other facts, or as to the amount of credit to be given to those statements, or whether the observations were general or confined to particular localities, take it as a fact *incontestably* proved, that the climate of Madeira has not, and never had, that effect upon invalids which both the public and the medical profession attribute to it. And, strange to say, in order to arrive at this *incontestable* conclusion, we must read those cabalistic numbers according to the interpretation of the author in question; for no one else, not even the three meteorological observers themselves, who, be it remembered, were members of the medical profession, were able to discover that they conveyed any such meaning, as the following quotations fully show.

Dr. Gourlay* writes as follows, except that I have underlined some words which, in the original, are not printed in italics.

At page 31, “The salubrity of the climate in this island, so highly extolled, is greatly attributed to the *uniformity of its temperature*. A regular succession of land and sea-breezes, cool and purify its atmosphere during the whole year, and, especially, during the hottest months. Hence, a drop of dew seldom falls,

* Observations on the Natural History, Climate, and Diseases, of Madeira, during a period of Eighteen Years. By William Gourlay, M.D., Fellow of the Royal College of Physicians, Edinburgh; and Physician to the British Factory at Madeira. London: 1811.

except in the higher parts of the island; and any deleterious effluvia, which may arise from the surface of the earth, or from other sources, are dissipated as soon as they are produced."

At page 32, "During the day, the whole range of the thermometer will seldom, at any season, exceed two, or at most four degrees, and frequently, for several days together, the same degree of heat is indicated.

At page 33, "Where such uniformity of temperature exists, combined with purity of atmosphere, and where such a pleasing variety marks the climate, one would conceive that the inconveniences of seasons would be unknown; and that neither the excessive heats of summer would molest, nor the colds of winter pinch, the frame; but it is found that this pleasing picture is not *entirely* realised; and though it may be truly said that, in general, spring and autumn compose the whole year, yet it is not to be concealed that, during the months of *July, August, and September*, which are the hottest months, the heat becomes excessive and intolerable; and that, on one or two occasions, the winter has been distinguished by a severe storm. Still, however, the winter may be said to be known only, perhaps, by a gale of wind, which may drive the vessels in the roads from their anchorage, or by a torrent of rain, which produces a rapid flow of the rivers down the ravines." It should be observed, that July, August, and September are not included in the Madeira season, that very few English invalids remain in the island during those months, and that those few usually remove to higher and cooler residences during the heat of the summer.

At page 90, "Madeira, from its *uniformity of temperature* and purity of atmosphere, has long been, and still continues to be, the favourite retreat of consumptive patients from the northern parts of Europe. Here, the unhappy sufferers under this formidable disease *cheat the winter of their own climate*, and gain that cessation of suffering which such a situation is fitted to produce."

At page 92, after describing the class of patients who had been ordered to Madeira, "Before such patients repair to this *last haven* of health, their malady is unfortunately, in too many cases, in its last stage; when neither change of climate, nor any remedy whatever, can be of service. From what cause this back-

wardness to an earlier trial of a southern climate proceeds, is not for me to determine; but it would be well if the physicians of such patients were to recommend a change of temperature in the first stage of the malady, where, etc.”

I have been unable to procure a copy of Dr. Heineken’s meteorological observations, but the following extracts from a letter of his, dated Nov. 1826 (the same year as that in which his meteorological observations were published) and printed in the appendix to Mr. Lyall’s book,* will answer my present purpose.

At page 334, “ Dr. Price estimated the expectation of a child at birth, in London, to be nineteen years of life, and Dr. T. Heberden, in Madeira, thirty-nine years. Without, however, stopping to inquire into the accuracy of these estimates, or the probable causes for their very great disproportion, allowing them to be correct; it is certainly true, that Madeira is remarkably healthy: from most of the diseases peculiar to warm climates, it is exempt; and many of those which in more northern latitudes, from the frequency of their occurrence, and epidemic or endemic characters, become a scourge, are here either altogether unknown or but slightly felt.”

At page 337, “ I shall take for granted, that my medical brethren in England will only advise those who are likely to benefit by climate to quit their native shores; and, with this proviso, I do not hesitate to say that Madeira holds out advantages that are not to be met with combined in any other quarter of the globe.”

At page 339, “ The temperature of Madeira is more equable (contrasting day with night, and summer with winter) than that of any other place. Our rains are violent, almost tropical; but they are also periodical and circumscribed, and never lingering and teasing. We are entirely free from the piercing keen winds which are met with, more or less, all over the continent of Europe, and enjoy, throughout almost the whole summer, although more partially than between the tropics, “ the trades ” and land and sea-breezes which there prevail.”

Dr. Mason says (page 37): “ The observations made at Sta. Luzia apply to that locality alone, and cannot in any way be made to suit the island generally; nor will they give precise

* *Rambles in Madeira and in Portugal.* London: 1827.

information relative to the lower part of Funchal, near the sea, especially as regards the progress of humidity during the day, although, in point of temperature, they may be very near the truth....I may also remark that almost every locality offers something peculiar alike with regard to temperature, humidity, or the local winds which prevail; and that until a series of observations shall have been made in different localities, the full merits of the climate, as regards the suitability to different diseases, or even stages of the same disease, will never be fully ascertained," etc. On the same page he states that invalids generally reside above the town, in the same line in which his observations were made. This may have been the case some twenty years ago, when Dr. Mason wrote, but it is very different now.

At page 133, Dr. Mason says: "From this statement (a long one, which it is unnecessary to quote), we can account for the fact that patients who visit Madeira are so differently affected by the precisely same conditions of the atmosphere; some experiencing relief, and others only an aggravation of their complaint. Accordingly the present work will be regarded, not as an attempt to prejudice that island as a resort for invalids, but as an effort to point out the danger of an *indiscriminate* reliance upon the sanitary effects of its climate. Such a reliance is injurious. The not unfrequently frustrated hopes of anxious friends, suggest advantages, real or imaginary, from a resort to other localities; whereas the atmospheric phenomena of Madeira being ascertained, and the requirements of the patients found to correspond therewith, comparative uniformity of success would establish its reputation; and the failure of cases to which its climate is not adapted, would not be attended with the effects of damaging its character as a residence for those who, by a change to such a locality, might reasonably calculate upon the realization of their most sanguine expectations."

Again, at page 152: "Those who, on their arrival, find the climate agrees with them, had better immediately remove to a drier climate; while those with whom it materially disagrees, as indicated by the symptoms which I have described, may rest assured that they will derive permanent benefit from remaining, that their hopes will not be blighted, but that returning health

and strength will result from leaving for a season their own less hospitable climate."

At page 162, writing on the climate of London: "In its effects on the animal economy our summer season will approach to the Madeira climate, being slightly modified by temperature and hygrometric condition."

Notwithstanding these opinions of Drs. Heineken, Gourlay, and Mason, it is now clearly ascertained, as we are informed by Dr. Burgess, that their own *meteorological observations* incontestably prove that the sanitary fame of Madeira is a pure delusion. The hieroglyphics which those gentlemen merely placed upon record have now been decyphered, and their meaning admits of no further dispute. It is ascertained, moreover, that Dr. Mason was mistaken in supposing that those which he found at Sta. Luzia Cottage were applicable to that locality alone.

The words "luxuriant vegetation", in the extract which I have quoted from the *Athenæum*, call for some remarks. The books which have been written on Madeira contain many statements on this point, which, without being more than commonly exaggerated, are calculated to entirely mislead a stranger as to the *general* character of the island. So far as my knowledge extends, there is scarcely a spot to be found where there is moisture sufficient to support luxuriant vegetation, without the assistance of artificial irrigation. Sir H. Davy, as quoted by Dr. Mason (p. 41), alludes to the great quantity of basaltic rock uncovered by vegetation. It is true that artificial irrigation is carried to a great extent; nevertheless the more general character of the surface of the country is dryness, barrenness, and absence of luxuriant vegetation. Even grass for horses and cows is brought down *daily* from the mountains; and in the summer, even on the mountains, the grass becomes so dry that a spark of fire will endanger the whole district. The whole island is of volcanic origin; its surface is either precipitous, or very much inclined, and the soil is of a porous nature. With the exception of a volcanic crater, and one or two other spots at distant parts of the island, none of which have I ever seen, I believe there are no hollows or basins capable of retaining water; and those to which I allude as the exceptions, do so only for limited periods. I have never seen a lake or even a pond.

There are some yam-grounds which, I should suppose, must be injurious to the health of those persons who live on or close to them; but they are quite unworthy of further notice in this place.

The lestes of Madeira are sometimes made great bugbears. We may judge of Dr. Mason's opinion of their *importance* to invalids, by the passage which I have already quoted, in which he advises those persons with whom the leste materially *dis*-agrees, to *remain* in the island. With respect to his advice that those who, *on their arrival*, find the leste agrees with them, had better *immediately* remove to a drier climate, I have to remark, that the season here is considered to begin on the 1st Oct., and to end on the 31st May. Now I arrived in the middle of September 1850, and it was not until some time in the following June that I had an opportunity of judging of the effects of a leste; and my experience of lestes during the three succeeding seasons has not been much greater. The precise and strong symptoms which Dr. Mason describes as distinguishing these winds, may occur sometimes, for anything that I know to the contrary; but most assuredly not always. It is not unfrequently a matter for doubt and difference of opinion, whether or not there is a leste; and the strong symptoms which Dr. Mason describes, I have never witnessed in the course of four *seasons* and three summers.

The principal point in dispute respecting the climate of Madeira, is, whether it is dry or damp. Persons who judge of it by the test of their own feelings, use such expressions as the following: "The air is soft and delicious, and strikes with a peculiar charm the stranger, whom, perhaps, a few days have transferred from the gloom and chill of an English winter." "The dry and balmy air which produces this never-ending spring, makes the step buoyant, and raises the hopes of the sufferer, who a few days before left the choking fogs, the rains and chilly damps, of the Thames and the Medway." Dr. Mason, however (page 33), says, "It would be a difficult task to convince many of the residents that the climate is at all damp, although the fact admits of being proved in the most satisfactory and philosophical manner." Let us admit, for the present, that Dr. Mason has proved in the most satisfactory and philosophical

manner that the air of Madeira is very damp, that is to say, damp in philosophical language; and let us suppose also, for the present, that in philosophical language a damp air is one that contains a more than average quantity of moisture, the measure being the number of grains of water in a cubic foot of air; for this, as I understand, is the manner of estimating the dampness of a climate to which Dr. Mason alludes. Are we to infer from these admissions, that persons who describe the climate, manifestly with reference to its effects upon the sensations, are labouring under a pure delusion? Is it not obvious, on the contrary, that the sensation of dampness depends upon something besides the number of grains of water in a cubic foot of air, and that Dr. Mason's satisfactory manner of estimating dampness fails to detect that not unimportant something, whatever it may be? Philosophers, in their arguments with the unlearned, frequently contrive to beg the question. So, in the present case, Dr. Mason (or his predecessors, it is immaterial which) first appropriates the words dampness and dryness, and, by implication, defines their meanings to be in accordance with his method of estimating those qualities. Then, secondly, he has no difficulty in proving the ignorance and obstinacy of all who persist in calling the air damp or dry, according to the original but less definite application of those words. And, thirdly, he absolutely ignores those differences in the state of the air which are not indicated by his method of estimating it. Nevertheless, I do not believe it has ever been ascertained, nor does Dr. Mason assert, that dampness, as measured by his method, furnishes a truer criterion than our own sensations do, of the *suitability* of a climate for animal life. His method has the advantage of being a definite and philosophical measure of something; whilst the other is vague and uncertain. But the philosopher has no right to rest satisfied with his method. There are other qualities of air, of which we have sufficient evidence; and it is his business to discover, define, and estimate them. It is most unphilosophical simply to ignore those other qualities, because they cannot, as yet, be defined or measured by any known instrument. Such distinctions as those to which I have just alluded would enter into theoretical speculations, but would be of little practical importance, at present, if medico-meteorologists would

always be content to deal fairly with the question. Dr. Mason tells us that his method of estimating dampness gives materially different, nay contradictory results, when compared with non-instrumental estimates; but he does not fall into the error of appropriating to a damp climate, as estimated by his method, all the ill effects which either experience or prejudice has attributed to dampness, as tested by the other means. For this latter step we are indebted to other persons, who raise the cry of vapour, moisture, luxuriant vegetation, dampness, etc., etc., in the ears of persons who are affected by a species of hydrophobia; then appeal to Dr. Mason and other authorities, for certain facts which answer the intended purpose, and carefully suppress the opinions of those same authorities, as to the value of those facts and the inferences to be drawn from them.

Dr. Mason tells us (page 18) that the mean temperatures of the air of London and of Madeira are about 50° and 68° respectively, and consequently that, when saturated, the air of the latter contains twice as much moisture as the air of the former. Another inference, which we are equally at liberty to draw, is that the air of London, when absolutely saturated with moisture, is as dry as the air of Madeira when in a state of only half-saturation. Now, people of ordinary understanding would certainly call the former excessively damp, and the latter excessively dry. Nevertheless, we must bow to our medical philosophers (not Dr. Mason, but others of more recent date), and not merely admit that, in such a case, there would be equal quantities of water in the air of both climates, but also that, so far as regards animal life in general, and all diseases in particular, which are usually considered to be much affected by dryness or dampness, the two climates are to be considered as on a par—that the air of London, when in a state of absolute saturation, is on a par with the air of Madeira in a state of half-saturation! Surely, this must be quite conclusive against the climate of Madeira, especially when we remember that a state of half-saturation corresponds to a depression of the dew-point equal to about 18° , whilst Dr. Mason allows us (see p. 26 and table xxxii) a mean annual depression of 7° at most. There is, however, another way of estimating the dryness or dampness of air, which is equally well known to the ordinary meteorologist; and I would

submit to your judgment whether it ought to be absolutely discarded in medico-meteorology. At moderate elevations, the air, whether in England or in Madeira, seldom is absolutely saturated with moisture. Consequently, if we understand the comparison between the air of those two places when in that state, to mean nothing more than the words literally express, the fact is of no great value. Perhaps we are intended to understand that, as a general rule, the air of Madeira would be twice as damp as the air of London. But, under the point of saturation, if the air of Madeira contains just twice as much moisture as the air of London, the former may still be considered, in another sense, twice as dry as the latter; that is to say, it is capable of absorbing twice as much *additional* moisture. Consequently, there are two methods of comparing climates: one of which might enable us to pronounce that climate A is twice as damp as climate B; whilst the other enables us to state just the reverse; and either would admit of being proved in a most satisfactory and philosophical manner. Such of our medical men as may wish to prove that the climate of Madeira is a mere delusion, and to advise their patients indiscriminately to remain in England, may adopt the former method; but they may perhaps find it convenient to be provided with the other, in case the question should be, a choice of residence in England or in the polar regions, where, undoubtedly, the air contains a very small quantity of moisture; since, if we take the average temperature of an extreme northern climate as high as 10° F., it will contain only one-fourth part as much moisture as the air of London, both being saturated.

The observation which I have quoted at the beginning of the preceding paragraph, is a casual remark of Dr. Mason, to which he attaches no more importance than it deserves: and, consequently, I should not have thought that it required to be noticed, if I had not seen that it is misused by the manner in which it is quoted, misquoted, and re-quoted elsewhere.

I shall now proceed to make some observations respecting the accuracy of Dr. Mason's meteorological observations, and their value as general results. I have learnt, from what I consider sufficiently good authority, that, during his residence in the island, Dr. Mason was several times absent from Santa

Luzia cottage, for many days if not weeks at a time. Indeed, he writes of having been at Santa Cruz. How his meteorological register was kept during those intervals, or by what process of calculation the blanks were filled up, we are not informed. We are only told, that he never for a moment deputed the task to any other person.

At page 1, he tells us that the height of Santa Luzia cottage, the place at which he made his observations, is 350 feet above the level of the sea, and at pages 82 and 83 it is stated at 300 feet. Having taken some pains to measure the height of my own house, which is only a few yards distant from Dr. Mason's house, I can state that the latter is less than 250 feet above the level of the sea, and not 350, or even 300 feet.

At page 2, he says the register-thermometer for external temperature *in the shade*, received the sun's rays *obliquely* from 2 to 5 P. M. At least, I so understand the sentence. However, he probably meant, that the stone pillar against which the thermometer was placed was so exposed; but either is very important: and, exactly what he means by the *oblique* rays of the sun, in this latitude from 2 to 5 P. M., and of course all the year round, I am at a loss to understand.

At page 3, he tells us that the direction of the wind was determined by a vane, placed upon a high staff. It is true that the staff, which still remains, is nearly 8 yards long; but it stands so much below the level of my house, and other buildings in its immediate vicinity, that I can only regard it as a useless toy. Even if the buildings were removed, the vane could be expected to indicate only the direction of the current of air in that part of the valley, and would be quite useless for general purposes. I confess I am at a loss to point out any systematic method of registering the direction of the wind, in this mountainous country, which would be at all satisfactory; but I consider Dr. Mason's register of his own wind-vane as mere waste paper. One might as well register the flickerings of a candle in a room.

A more important question is, the value of Dr. Mason's hygrometrical observations. Santa Luzia cottage is a very small house. The room in which the hygrometer was placed opens towards the garden, which is much confined by high walls, especially near to the house. The room is supported upon stone pillars, with

an open area under it and for a little space in front of it; the garden being two or three feet above the level of the area. Close to, and nearly on a level with the room in question, is an open water-tank, the vent of which leads into the area. The leakage and waste water would naturally keep the area constantly damp, and, when I saw it, such was the case. An open water-course, called here a lavada, runs across the garden at the distance of a few feet from the windows of the room in question, and quite close to the area, in fact upon the low wall which forms the limit to the area on the garden side; and this lavada, being two or three feet higher than the area, would tend to increase its dampness, both by surface-evaporation and by filtration. The garden, I am told, was in a state of luxuriant vegetation, and grew, amongst other things, bananas. When I visited the room, I immediately perceived a damp, mouldy smell, which I presume arose from the area and tank, under and in front of it. The following is what Dr. Mason himself says with respect to the tank (see page 40). After speaking of the tanks kept to irrigate gardens, "I feel fully justified in attributing to those sources the effect of poisoning the air; as I suffered severely in my own person all the symptoms generally referred to the effects of marsh effluvia—such as extreme lassitude, pains in the head and limbs, intolerance of light, mental depression and anxiety, dry, parched, brown tongue, etc.—all which disappeared in three days, without the aid of medicine, upon removing to Santa Cruz, a few miles from Funchal. On my return to Santa Luzia, the same symptoms re-appeared after a residence of a few days, and continued, unabated, till this source of annoyance was partly removed, when some amelioration of the symptoms took place. I have not the least doubt that they would have disappeared completely, could the stagnant water have been entirely got rid of; but, although my landlord had lived some years in England, I had much trouble to convince him that water could be at all offensive, after being kept two months in a tank."

From the latter part of this quotation it seems probable that, during a portion of Dr. Mason's residence, the tank was in a worse state than it is at present; and this is somewhat confirmed by the description which the late Mr. Wilkinson gave me of it;

since his description is barely justified by the present state of the tank. Mr. Wilkinson also told me that he frequently remonstrated with Dr. Mason, on the absurdity of placing his hygrometer in such a situation as the one he had chosen. On this point, however, Dr. Mason says (page 3), "The hygrometer was situated in a room to the west, between two windows constantly open from 6 A. M. to 6 P. M., and very free from currents of air, as they acted like folding doors. This room was, consequently, free from local humidity arising from the evaporation of water from the ground, etc." Can anything be more absurd than to suppose open windows, acting like folding doors, would exclude local humidity? What can be the value of hygrometrical observations with reference to the general climate of Madeira, which were dependent upon such means of excluding peculiar local influences?

At page 6, Dr. Mason says, "There is a striking coincidence in the results afforded by Dr. Heineken's observations [made eight years before those of Dr. Mason] and my own, although the instruments by means of which they were obtained are so widely different; proving that when such observations are based on facts, instruments, however varied in their constructions, must offer, upon comparison, results which accord with one another; because nature is uniform in her operations; whence the same causes invariably produce the same effects." At page 203, the editor presents us with a comparison of Dr. Mason's and Mr. McEwen's observations made with similar instruments in the same months of different years, the latter giving upon an average somewhat more than double the degree of dryness stated by the former, and accompanied by the following remarks: "The difference of locality, or the circumstance of his (Dr. Mason's) observations having been made in the house, with open windows, and mine (Mr. McEwen's) out of doors, does not sufficiently explain the discrepancy, which I think fully proves—what Dr. Mason suspected—that the different years vary much more than is generally admitted." How very consistent are these two proofs! It may be as well to remark also that the results given by Dr. Mason, with respect to the mean annual dryness on the dew-point hygrometer (to use his own expression), at nearly the same hours of the day, are as follows: Dr. Mason's,

7°.42 — Dr. Heineken's, 7°.42 — a very striking coincidence certainly, since there is not a difference of even one-hundredth part or a degree. However, since Dr. Mason's observations were not made with the dew-point hygrometer, his result, when referred to the dew-point, depends upon the factor of reduction which he employed. Now Dr. Mason employed a constant factor, without regard to the temperature of the air; but if we employ Mr. Glaisher's factor (taking the temperature of the air between 65° and 70°, since it is stated in Table xxvii at 68°. 12) the results will stand thus—Dr. Mason's, 5°. 41—Dr. Heineken's, 7°.42—the latter being 37 per cent. greater than the former. I have already pointed out, in the comparison of Dr. Mason's and Mr. McEwen's results, obtained with similar instruments, that the last is 100 per cent. greater than the first. Whether the facts on which these several results are based are erroneous, or whether nature has deviated from the ordinary uniformity of her operations, are questions which I shall leave for future investigation.

At page 31, Dr. Mason says, "In order to prove the dampness of the climate, I may instance the impossibility of keeping iron, in any form, from being rapidly oxydized. The different powders, such as opium, squills, etc., soon lose their pulverulent form, and become firmly united into a solid mass; various neutral salts rapidly deliquesce; gloves, shoes, etc., soon become covered with various species of cryptogamous plants; silks become spotted and unfit for use; pianofortes frequently require retuning; and the screws of various other instruments, as violins, guitars, etc., became so tight as to be almost immoveable. In fact, it would be impossible for vegetation to flourish, were not the atmosphere almost saturated with moisture; as frequently, during the fine season, there is scarcely a shower of rain for three, four, and sometimes even six months in succession." To take the last statement first, I do not believe that vegetation does or could flourish, during such seasons as Dr. Mason describes, without artificial irrigation. It is somewhat remarkable, moreover, that Dr. Mason should speak of such long periods of drought, since, in his table vi, there is not one month in which rain did not fall on two days and two nights at least; and, on those occasions, it rained during the whole of the two days, and only two hours

short of the whole of the two nights. The other facts I can easily imagine Dr. Mason really observed in his own house, since I have heard of similar occurrences in other houses: but I can make a counter-statement. I have now just completed my fourth season of residence in a house which is the next above Santa Luzia cottage, and only a few yards from it. Gloves, boots, and silk handkerchiefs, kept in my own room during the whole of those periods, have never shown the slightest symptoms of becoming mouldy or spotted. I have a great number of iron and steel tools, which have remained constantly in another room, ever since my arrival here nearly four years ago. Though I do find that the tendency of iron to rust is greater than I have observed in inland places in England, I much question whether it is at all greater than at many sea-side places in England. Some of the polished steel tools, which have been kept nearly four years in the same room, without being either used or cared for, remain to this day without even a minute spot of rust, so far as I have observed from casual inspection. With respect to the statements that the screws of violins, etc., become tight, I should attribute such a result, not to the general dampness of the climate, which I think could scarcely have that effect, but to the removal of the instrument from a dryer to a damper situation. The screws being properly tight when dry, might become almost immovable when swelled by moisture. Consequently, it is easy to believe that the screws of violins, guitars, etc., always become fixed when taken into Santa Luzia cottage. Though I have not been in the habit of keeping a meteorological register, I have frequently observed the hygrometric state of the air—not with great care nor with very good instruments. The results, however, when compared with Dr. Mason's, are far from proving that “nature is always uniform in her operations.”

Admitting that Dr. Mason's meteorological observations are entitled to more credit than I give to them; yet, in a paragraph which I have already quoted, we have his own statement that his results cannot in any way be made to apply to the island generally, and that, until a series of observations shall have been made in other localities, the merits of its climate can never be fully known.

I cannot take leave of Dr. Mason without alluding to one

other circumstance.—A paragraph in the book to which I have already alluded has been pointed out to me by a lady. It mentions Dr. Mason's fate as "a telling comment on the blind credulity which prevails respecting the virtues of *foreign* climates in pulmonary consumption:" and then follows a quotation from the editor's preface to Dr. Mason's book, which, I confess, does not appear to me to furnish any such comment. But it is a melancholy and pathetic tale, and, since it seems to have touched the too sensitive feelings of the gentleman in question, it would be illiberal to find fault with his logic. Nevertheless, he can scarcely complain, if I merely contribute a little more pathos, by quoting the very first paragraph of the same preface. It is as follows, except that I have underlined some passages which, in the original, are not printed in italics.—"Apart from the value of Dr. Mason's work as affording a just estimate of a climate," (which, by the bye, both the author and the editor afterwards tell us it does not do) "the resort of a particular and large class of invalids; his labours acquire an interest from the fact of their having been prosecuted in a state of extremely infirm health, *regardless of the influence which they must have had in aggravating the symptoms, and lessening the chance of recovery.* He may truly be said to have *sacrificed his life to professional zeal.* Contending with an extensive derangement of the pulmonary functions, he resolutely *cast aside all solicitude* for his own health; and, without intermission or pause, completed a series of difficult and fatiguing observations, with the noble view of rendering a benefit to society. The *exposure* and *privations* which he would have imperatively prohibited a patient from encountering, he fearlessly and enthusiastically contended with in his own person; undeterred by the most *trying fluctuations of temperature*, the prostration attendant upon a constant strain of mind, and the watching which broke in upon that ordinary rest which even the robust cannot forego without some degree of suffering. To none would he, for a moment, depute the task which he had undertaken; and, when all around him were enjoying repose or courting it, this *martyr*, as he may be called, to *meteorological investigation* passed the night *with his instruments and journal*, noting down the minutest changes which the atmosphere underwent, from the first sinking of the sun to

the first indication of its rising.” Alas ! what a melancholy instance is this of blind credulity respecting the virtues of foreign climates !

I do not wish the preceding remarks to be understood as indicating any opinions of my own, as to the sanitary effects of *foreign* climates in general, or of the climate of Madeira in particular, with reference to English invalids. It is a question upon which I feel totally incompetent to form an independent opinion of any value. My object has been to expose the insufficiency of the grounds upon which other persons have arrived at conclusions which they have not hesitated to pronounce with so much self-confidence. I have not asserted that their conclusions are false ; but I dispute the accuracy of the data, and the validity of the reasoning by means of which they profess to have arrived at those conclusions. Neither do I wish to assert, as a fact really ascertained, that Dr. Mason’s cottage was so damp or so ill-chosen a place for hygrometrical experiments as some of my observations might seem to imply. It is sufficient for my purpose, if I have shown what I believe to be the fact, namely, that so much suspicion attaches itself to the value of his hygrometrical statistics, that no reliance ought to be placed upon them. Nevertheless, it may be as well that I should now state what my opinions are with respect to Madeira, if only for the purpose of preventing a false inference being drawn from my silence on that point. In a few words then, my opinion is, that the climate is extremely beneficial in many cases. That the cases which are likely to derive benefit from the climate can, in general, be discriminated by the resident medical men ; but that, owing partly to the effects of the climate being less well understood by medical men resident in England, many invalids are sent here who had much better have remained at home. Moreover, there are two questions which ought to be carefully distinguished : the one, whether the climate is calculated to act beneficially on any particular disease ; and the other, whether it is advisable to send a particular individual who is afflicted with that disease to this climate. For example, the same author will tell us, on one page, what a melancholy sight it is to see poor deluded individuals seeking for health abroad, alone, and away from their families and the comforts of a home ; and, on

another page, of the folly of individuals going to Italy in search of health, and spending their time in cathedrals, picture-galleries, theatres, ball-rooms, etc. : two somewhat contradictory representations, if each is to be understood as applicable to the class invalid in general ; both of which are, however, put forward as valid arguments against the abstract climate theory ; though the latter, so far as it is worth anything, furnishes an argument in its favour : for the same persons neither would nor could have spent their time so agreeably in England. That such excesses should induce greater evils than the climate can compensate for, is but too probable ; but such a result furnishes no argument against the *climate theory*. The abuses of climate, which are very frequent and well known to resident medical men, are too often overlooked or not fully appreciated by medical men in England, when they have to consider the advisability of sending a patient abroad ; and also, when they attempt to form opinions respecting foreign climates, by means of the results to those whom they have sent abroad, and who, of course, are almost certain to conceal or misrepresent every imprudence of which they have been guilty. For myself, I have reason to think that I owe my life to the sanitary effects of the climate of Madeira. I do not, of course, forget the benefits which I have also derived from medical advice, and the strictness with which I have always acted upon it.

I have expressed an opinion that the science of medico-meteorology is very imperfectly understood. I am not qualified, nor can I afford sufficient space to enlarge much upon this point ; but I shall, nevertheless, hazard a few observations, even at the risk of exciting the wrath of some pseudo-scientific members of your profession, who make many words, not in the spirit of scientific inquiry, but in that of arrogant dictation, upon a subject which, it is evident, they do not understand—which, in fact, is not understood by any one—and by them not sufficiently to enable them to see their own ignorance.

I fully admit, as a general rule, that the warmer the climate the greater is the quantity of aqueous vapour contained in a cubic foot of air. Is there any reason for doubting that this is a beneficial provision of nature ? In any given climate of limited extent, a particular spot which is more than ordinarily damp, as

measured by the proportion of aqueous vapour to common air, is usually found to be unhealthy. But if we attempt to compare places situate in very different latitudes by the same rule, it leads to results which are so preposterous that the method of comparison is obviously inapplicable. I do not believe the quantity of pure aqueous vapour in the air is a matter of nearly so much importance as it is generally supposed to be ; but, that many injurious effects upon the human system, which are attributed to dampness, depend, if not entirely, at least in great measure, upon other elements. It may be that *moisture* causes those other elements to be developed, or that *aqueous vapour* acts as their vehicle. It may be that the same causes often produce both dampness and those other elements. On either of those suppositions dampness might be mistaken for the cause of the effects observed, especially when we have no precise means of detecting any other cause to which they can be attributed ; and I cannot help thinking that such mistakes are frequently made. Can it be contended that the air of a close, dark, damp cellar is no more injurious to human health than equally damp air rendered so by fresh steam from a tea-kettle, or other similar means ? We know that over marshes miasmata sometimes exist to such an extent as to depopulate large districts, whilst a sea-air, more than equally damp, is considered conducive to health ; yet no one doubts that the miasmata are caused by the moisture of the marshes. Though the existence of miasmata in marshy localities is fully recognized, their presence cannot be detected by any meteorological instrument, or even by chemical analysis of the air ; and consequently it is easy to believe that we cannot form correct estimates of the salubrity of air by any instrumental or even chemical investigation of its qualities. I feel a strong conviction that many effects which are attributed to aqueous vapour alone, are, in fact, produced by other constituents, which frequently, but by no means necessarily, accompany it ; and that whether aqueous vapour is or is not accompanied by those constituents, depends upon the source from whence it is derived ; perhaps somewhat in the same manner as water acquires different qualities by filtration through different mineral strata ; and that it is as great a mistake to attribute those effects to the aqueous vapour, as it would be to attribute the peculiar effects

of different mineral waters to the one constituent water. A slight excess of aqueous vapour, if it happens to be derived from a poisoned source, may be a matter of great importance; and within a limited district, a slight excess of aqueous vapour at a particular spot, often does arise from such a source; and, in other cases, it is an indication of want of free circulation of the air, and consequently of an undue accumulation of noxious vapours. But no such inferences can be drawn from comparisons of the quantities of aqueous vapours in the air of places at great distances from each other; in which case the difference in the quantities of aqueous vapour is likely to be dependent upon more extensive and essentially different causes.

I do not mean to assert that aqueous vapour is an unimportant element in medico-meteorology; but, until the many other elements which enter into the question can be more certainly detected, and their effects eliminated, I think that hygrometry will be of very limited practical use. It must not be forgotten that there are many recognized meteors, such as electricity, ozone, and various gases, which can be estimated, though with more or less difficulty, the effects of which upon the human system may be considerable, and that as yet very little is known about them. What Dr. Prout says in the appendix to the *second edition* of the eighth number of the *Bridgwater Treatises*, appears to me to deserve the attention of medico-meteorologists. I refer to his suggestions respecting the opposite effects of pure aqueous vapour, and of aqueous vapour in union with oxygen or deutoxide of hydrogen; the affinity being apparently so slight, that this combination occurs only when the oxygen in the atmosphere exceeds the chemical equivalent, 1 of oxygen to 4 of hydrogen; and that the results of every *common* analysis and examination of air are the same nearly as if such a state of combination did not exist. With respect to temperature, I have no remark to make, except that we must not lose sight of the fact, to which you will strongly testify, namely, that it is not merely the general or average state of the air that determines the salubrity of a climate, but that the amount and suddenness of the changes to which it is liable are most important points, especially for invalids. Of course we ought also to take into account the question, how far particular changes, or injurious influences, necessarily

affect an invalid, and how far he may be protected from them by reasonable care. In which view of the matter, habitual heavy dews for an hour about sun-set, or even during the whole night, might be regarded as of no moment; whilst a liability to sudden changes of temperature, or a hot sun and cold winds prevailing during the day, would be extremely injurious. The equability of the climate of Madeira is, perhaps, one of its chief virtues. It would, however, be a mere delusion to suppose that it is not liable to changes. There can, I imagine, be no place on the face of the globe which is not liable to changes, some more and some less than others. It would be equally a delusion to suppose that the climate of Madeira is a specific cure for consumption; and writers on climates might, perhaps, spare themselves the trouble of proving that it is not so. No sensible person, who understands what those words mean, can for a moment entertain such an idea. I must return to my starting-point, and restate my opinion that, except in some extreme cases, the sanitary properties of climates can as yet be determined only by their sanitary effects. For data of this description we are dependent, in great measure, upon the opinions of medical practitioners, and none can be so good as those of the medical men resident at the place in question. Dr. Burgess may say, or, by quoting the editor of Dr. Mason's book, may imply, that the resident medical men being prejudiced and interested, their opinions are not to be trusted; but, in so saying, he libels the profession in general rather than the individuals in particular against whom such observations are levelled. If we are to cast aside all medical statements and opinions which are open to the same objections, what have we left to trust to, and whom shall we believe? We are not, however, bound to accept either their assertions of facts, or their opinions, absolutely in the dark. There are various means of sifting evidence and ascertaining its value, and we are furnished with a great deal on the subject of climates which will not stand the test.

When I began this letter I had no intention of making frequent reference to Dr. Burgess's book, which I had not read. By degrees I have been drawn into an examination of the Madeira portion of the first chapter, comprised in ten pages; and I must present you with some of the fruits of my investigation.

At page 11, Dr. Burgess quotes from Mr. White a statement respecting the equability of this climate, and then tells us that Mr. White *reluctantly* admits that, although so very equable, the climate is not altogether free from changes, which constitute there, as elsewhere, the exciting causes of pulmonary affections. Mr. White's words are, "pulmonary and inflammatory affections", immediately followed by these words, which Dr. Burgess also omits: "These, however, as may be supposed, are comparatively rare among the better classes, and occur chiefly among the hard-working poor, who are more exposed to this cause and to sudden chills of the surface while perspiring profusely." Why did Dr. Burgess omit this sentence? A little further on Dr. Burgess says: "Yet in this most perfect climate, the same writer informs us that the different eddies or currents caused by the vicinity of the mountains, render either a vane or anemometer of little use." A wind-vane, or anemometer, in order to be of use, must be exposed to the wind; the valley of Funchal is sheltered from the wind on the north, east, and west sides, and consequently these instruments are of little use. It is futile to employ instruments for the purpose of registering the direction or force of mere eddies and currents of air, which are to be found in every sheltered spot which is not too confined; and the valley of Funchal has the advantage of being very extensive.

At page 12, Dr. Burgess writes: "It is also stated" (by Mr. White, I presume,) "that the position of his (Dr. Mason's) instruments was not well chosen"; and coolly continues, "However, a summary of Dr. Mason's observations will enable the reader to form his own opinions as to their merits, and the reliance to be placed on them." I should have supposed that a reader who wished to form an opinion as to the reliance to be placed on the accuracy of meteorological observations, would find it necessary to examine them in detail, and not by means of a summary. Moreover, Dr. Burgess does not give us a summary of Dr. Mason's observations, but merely a few *extracts* from the editor's preface, and from the *text* of Dr. Mason's work; and with what degree of fairness those extracts are made, I am about to show.

On the same page Dr. Burgess says: "Dr. Mason, who ultimately fell a victim to phthisis, went to Madeira with the belief that

he would recover his health under the alleged sanitary and benign influence of the climate of that island." I should like to know upon what authority Dr. Burgess makes this assertion, for Dr. Mason's editor tells us that "his visit to Madeira, and consequent residence there for a period of nearly two years, were purely accidental"; and then goes on to relate that Dr. Mason started for Nice; that "this object, however, was unfortunately frustrated" by his *disregard of self*, and devotion to a sick relative, and that he then came to Madeira; that Dr. Mason afterwards made a second attempt to reach Nice, "the climate of which, he had always been persuaded, was far better adapted to his case."

At page 13, Dr. Burgess quotes a paragraph from Dr. Mason respecting the hygrometric condition of the climate of Madeira, in which he (Dr. Mason) asserts that it is saturated with moisture during the greater part of the year, and then proceeds as follows (see page 14): "The author (Dr. Mason) supports this statement by a series of tables, from which it would appear that at the temperature of 50° , which is near the mean temperature of London, the air, if saturated, is capable of holding 100 parts of moisture in solution; while at the temperature of 68° , which is rather above the mean temperature of Funchal in Madeira, it will contain 200, or nearly double what it is able to hold in London." Judging by this paragraph, I presume that Dr. Burgess does not profess to have any knowledge of the science of meteorology; for I cannot imagine that any person at all acquainted with the rudiments of that science, could write such a sentence. First, Dr. Mason (see page 18), does *not* state that the air of London is capable of holding 100 parts of moisture, *except* upon a previous supposition, which Dr. Burgess omits, and thereby converts that statement into unintelligible nonsense. Secondly, to represent the deduction as one which appears from Dr. Mason's *series of tables* respecting the meteorology of Madeira, indicates a total misapprehension of the whole subject. Dr. Mason makes out his statement, not by his own tables, but by quoting from Mr. Daniel a physical fact in the science of meteorology. Thirdly, to suppose that Dr. Mason intended to prove, or even to support, his statement that the air of Madeira is saturated, by pointing out that *if it were* saturated it would contain twice as much moisture as the air of London, is too absurd. Is

this a specimen of Dr. Burgess's interpretations of meteorological observations, by virtue of which it is incontestably established that Madeira is no exception to the rule which he lays down respecting *foreign* climates? Doubtless Dr. Burgess has devoted his time to his profession, its studies and its duties; and, if he has not found leisure to make himself acquainted with the collateral science of meteorology, he is in the position of the majority of his professional brethren, and of other professional men, who, for want of time, must make some similar omissions. This may be a valid excuse for his misapprehension of Dr. Mason's statements, but hardly for putting them together in a manner which is equivalent to gravely telling us that a certain quart measure is brim full, and *supporting* that statement by reference to a *series* of tables from which it would appear that a pint measure, if full, is capable of holding 100 parts of water, whilst a quart measure will contain 200 parts, or double what a pint measure is able to hold.

Dr. Burgess, at page 14, writes, "By referring to the tables of Dr. Mason having reference to this matter, it appears that the maximum dryness observed during the leste [*the leste*] is $22^{\circ}.5$," etc. Dr. Mason's statement, see page 26, is, "By referring to the tables, it will be seen that the maximum dryness observed during a *leste*, etc." [*a leste*]. And, at page 28, obviously with reference to the same leste, or *African blast*, as Dr. Burgess would have us call it, Dr. Mason informs us that the dryness experienced during the *strongest* leste he had observed had been *equalled at Paris*. I confess I was surprised to find that Dr. Burgess had taken the trouble to examine Dr. Mason's tables, and to make deductions on his own account, but the mystery disappeared, when I discovered that the whole paragraph to which I have just referred, and which Dr. Burgess gives us as if it were his own, was, in fact, copied from Dr. Mason's book, with the exception of an insignificant (?) substitution, of *the* for *a*. We have other specimens of *insignificant* mis-quotations.

At page 15, Dr. Burgess thus writes: "The following observations with reference to the variability of the weather at Madeira, will perhaps surprise the reader: 'The very frequent and remarkable variations, in a given series of years, incontestably prove that Madeira is no more to be relied on than any other

place, for certainty of fine weather, and that it has equally its annual variations of temperature.' ” Truly, the reader ought to be surprised, if he gives credit to Dr. Burgess' quotations, that such an extraordinary statement should be made by a person who had resided nearly two years in this island. But his astonishment will, perhaps, take a different direction, when he is informed that Dr. Burgess has *cut out* ten words from the *middle* of the short paragraph which he has quoted, and that those words reverse, or, at all events negative, the meaning of the paragraph as given above. Dr. Mason's statement, see page 35, is : “ The very frequent and remarkable variations, in a given series of years—providing the ordinary observations of the inhabitants be strictly” [*strictly*] “ correct—incontestably prove, etc.” And this follows some remarks by which Dr. Mason *ridicules* the manner in which other people bring forward the testimony of the oldest inhabitants to prove, that particular seasons were variations from all former experience, and tells us, in effect, that such testimony is *not* strictly correct. Consequently, Dr. Mason does *not* assert any such fact as that which Dr. Burgess gives on his authority, and, professedly, in his words. And what Dr. Mason does say, admits of being understood to *imply* exactly the reverse. Are you surprised? I am not.

At page 16, Dr. Burgess writes, “ We have already noticed that, during the prevalence of the leste, or sirocco of Madeira, the air is excessively hot and parching. Within twenty-four hours after this wind has ceased, there is a copious fall of rain; and the author [Dr. Mason] has observed a very strong precipitation of dew three hours afterwards; the thermometer being reduced from 17° to 7° of dryness on the hygrometer, and at seven the following morning, to 2°, while plants and shrubs were covered with dew. Thus we find, a few hours after the leste has ceased, the whole atmosphere, from being intensely dry, becomes surcharged with humidity.” Of these three sentences, the first and last are, I believe, Dr. Burgess's, and the middle one is an extract from page 48 of Dr. Mason's book, but with the following alterations. Dr. Mason says that rain *generally* falls within twenty-four hours after the leste *has altogether ceased*, and he does not use the word *copious*. Moreover, it happens that, except on the mountains, no rain followed the

particular leste to which Dr. Burgess has thus called our attention, as will be seen by referring to the extract from Dr. Mason's journal given at page 194. And this was, moreover, the strongest leste that occurred during his residence in the island. Further, in the course of my experience, which is double that of Dr. Mason, no rain has followed any marked leste, within such a length of time as to connect the one phenomenon with the other; and I have no recollection of rain having ever so followed any other leste, though I am aware that we are sometimes told by the inhabitants that it is to be expected.

With respect to the first and third sentences, which, as I have already stated, are Dr. Burgess's, they refer to the former statement respecting *a* leste—which Dr. Burgess has chosen to write *the* leste—as if it were a fair representation of what always, or at all events usually occurs, when that wind blows; though Dr. Mason, upon whose authority these statements are made, speaks of *a* leste, and refers to a table (table xxv), in which he gives the maximum dryness of six lestes as follows:—January, 9° ; February, 9° ; March, 14° ; June, 15° ; October, $22^{\circ}5$; December, 13° . So far with regard to the fairness of Dr. Burgess's selection of statistics. Now let us see how far even they bear out his assertion, that the *whole* atmosphere, from being *intensely dry*, becomes *surcharged* with moisture. First, Mr. Glaisher tells us that, in England, the dew-point is sometimes 30° below the temperature of the air. Consequently, the fact that the wet bulb of the psychrometer was 17° , or even $22\frac{1}{2}^{\circ}$ below the temperature of the air during a leste, does not indicate a very extraordinary degree of dryness; since, if we take the temperature of the air at 80° , the corresponding depressions of the dew-point, according to Mr. Glaisher's tables, are $25\frac{1}{2}^{\circ}$ and 33° respectively. Also, the corresponding quantities of aqueous vapour in a cubic foot of air are 4.69 grains and 3.54 grains respectively; whilst the air of London, at the mean temperature of 50° , *if saturated*, would contain only 4.28 grains. Secondly—How does Dr. Burgess arrive at the conclusion that a few hours after the leste has ceased, the whole atmosphere becomes *surcharged* with moisture? Does he infer this from the fact that *dew* was precipitated, or from the fact that the so-called Mason's hygrometer never shewed a depression of less than 2° ; which, according to his

table XXXII, corresponds to $4\frac{2}{3}^{\circ}$ on the dew-point hygrometer : for Dr. Mason does not say on *the* hygrometer, but on *my* hygrometer—a variation which, to Dr. Burgess, probably appeared immaterial. As to rain generally falling after a leste, supposing it to be a fact—what then? We have in England heavy showers of rain—thunder-showers—within very much less than twenty-four hours after most oppressive heat. I must say that, so far as my experience goes, I think our total exemption here from that oppressive electric state of the air which, in England, often precedes a thunder-storm, fully compensates us for our occasional “African blasts.” Lastly, I must add what Dr. Burgess omits to tell us, namely, that during the leste selected by him as a fair example with respect to dryness, though it was the strongest ever experienced by Dr. Mason (see p. 194), the maximum temperature of the air was 81° F., which is *less* than the maximum usually attained during the summer in some parts of England.

At page 17, Dr. Burgess says, “Madeira seems to have no more immunity from disease than other places. Dr. Heineken and Dr. Gourlay both agree that no disease is more common amongst the natives than pulmonary consumption, and Dr. Mason corroborates that view.” Dr. Heineken has given an opinion which I have already quoted, that Madeira *does* enjoy much greater immunity from diseases than other places. Dr. Gourlay perhaps disagrees with Dr. Heineken upon this point, since he enumerates several causes, *peculiar to the natives*, which produce disease amongst them. How far these two physicians agree with respect to the prevalence of pulmonary consumption, I have not the means of ascertaining; but the following extract from Dr. Gourlay, page 90, puts his opinion in a somewhat different light. After referring to this island as the favourite retreat of consumptive patients from the northern parts of Europe, he adds: “Yet still, though so highly beneficial in this disease, with the natives of other countries, it is not to be concealed, that no malady is more prevalent here than phthisis with the natives of the island.” Why did not Dr. Burgess quote Dr. Gourlay’s opinion whole and entire, as he expresses it? He might have added, also, some extracts from the 5th chapter of Dr. Gourlay’s book, which enumerates some of the reasons

(which might be greatly increased in number) why the natives are subject to disease—reasons which in no way apply to invalid visitors. Dr. Mason probably knew very little about the matter; and he only ventures to say, that he *should be inclined* to corroborate Dr. Gourlay's opinion, that consumption and scrofula are frequent in Madeira: which means, only, that he would do so if he could.

I must now apologise for some portions of this very long letter. In the course of writing it, I have been somewhat drawn aside from my original purpose, and I must now state that I by no means wish my numerous criticisms of one writer in particular to be taken as having any general application, or as indicating my opinion of any other medico-meteorological writer whatever. I ought, no doubt, to revise what I have written, and render it more strictly consistent in its several parts; but you know that I am on the eve of a voyage to England, after a very long absence, and cannot possibly find time to do so. For the same reason, I am compelled to omit some remarks which I intended to make on the great and peculiar difficulties of making meteorological observations in this island as compared with England—partly with a view to point out how much must depend upon the judgment and discretion of the observer.

I shall conclude with one or two specimens of reasoning, which appear to me to be more than commonly loose.

Of what value is the argument that, in the course of a few generations, the human system becomes acclimated, and therefore, that the climate in which an individual and his ancestors have been born and lived, must be the best suited to that individual? Admitting this to be true as a general rule, surely persons who are afflicted with organic disease *may be* the exceptions, and, whether they are or not, should be determined by observation not by theory.

Dr. Burgess thinks that it is inconsistent with the laws and operations of nature that the country in which an individual was born, reared, and previously enjoyed good health, should be *no longer* suited for him when afflicted with organic disease. I suppose we must yield to Dr. Burgess what he appears to assume as a point too clear for dispute, namely, that the country in which an individual was born, reared, and enjoyed good health *down*

to a certain period, and then became afflicted with organic disease, is thereby proved to have been, of all climates in the world, the one which was best suited to his constitution *down to that period*. Still I cannot help thinking that another person who adopts as an *à priori* theory, that different climates may not improbably be suited to the same individual in two totally different states of his bodily health, proves himself to be possessed of an equally clear insight into the laws and operations of nature. Secondly, I should like to be informed whether it can fairly be predicated of the generality of persons who are afflicted with organic disease, that they were born and *reared* in the enjoyment of good health, and totally unaffected by disease down to the period which the word *reared* may imply. Thirdly, if that is to be answered in the affirmative, are there not, at least, a great number of persons who are afflicted with organic disease from their birth, or during infancy, or at all events before they reach the stage of life to which Dr. Burgess refers? Fourthly, would it not tend to clearer conceptions of the matter, if such statements as the one which I have last quoted, were made in somewhat more logical form, so that we might see, without reading every sentence two or three times over, what are the premises assumed as the foundation of the argument, and understand the nature, value, and extent of the conclusion. I feel sure that no one who, placing some degree of confidence in the author, reads such an argument only once, and with ordinary attention, can have a just notion of the course of reasoning through which he is led, as it were blindfold, and consequently at the risk of being misled. Fifthly, since the author in question thinks, as he tells us elsewhere, that the laws and operations of nature do not render it necessary for an invalid to be confined to the place in which he was born and reared, but that a proper locality should be selected within the limits of his own country, he ought to state whether England, Scotland, and Wales, are to be considered as one or as several. We understand that it is distinctly prohibited to an invalid native of Dover to cross over to Calais or Boulogne, and *vice versâ*; but we are left somewhat in the dark whether the Tweed forms a boundary line in Dr. Burgess's medical map of climates. Apparently the charm lies in the adjective *foreign* climate; but in a matter of so much importance to the public, the author ought to be more explicit.

With respect to Madeira, however, we have no reason to complain of want of explicitness. It is sufficiently obvious that Dr. Burgess regards it, in comparison with England, as an *extreme* climate. But upon what grounds? We may presume that he never placed himself within reach of the "African blast", and consequently that all his information is drawn from other sources than personal knowledge or experience. Respecting the manner in which he interprets, understands, and represents those authors whom he cites as authorities, and from whom, it is fair to presume, he has derived the principal part, if not the whole, of his information, I need make no further remark. I hope and believe that I have furnished you (not so much for your information as for your use) with better, fairer, and safer means of forming your own opinions of the merits, and of the reliance to be placed on his book, so far as regards the climate of Madeira, than I should have done if I had followed his example, and merely given you, under the name of a summary, a few extracts from what he has written on that subject, taking to myself, of course, as much liberty of interpretation and representation as he has thought himself justified in making use of with reference to other authors.

Whatever benefits Madeira may confer on invalids, in the shape of change of climate, it is somewhat remarkable that most authors, except Dr. Burgess, dwell a good deal on its mildness, and speak of the benefit it confers by enabling invalids to *avoid* the extreme changes of our English seasons; or, as Dr. Gourlay expresses it, "to cheat the winter of their own climate". If, however, people imagine that the valley of Funchal is a hot-house or a green-house, where even an eddy or current of air is not permitted to ruffle the leaves of a tender plant, they greatly deceive themselves; for, though we never have those severe, cold winds to which many parts of the Continent are liable, yet moderate breezes, amply sufficient to renovate and purify the air, are by no means uncommon.

Believe me to be yours most sincerely,

J. M. BLOXAM.

GEORGE LUND, Esq., M.D.